

## THE PRESERVATION OF WELL KNOWN BINOMIALS

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The International Code of Botanical Nomenclature, as revised at Cambridge in 1930 and further amended at Amsterdam in 1935, is now followed by all working taxonomists in America. Nevertheless, there are certain facts about the Code and certain principles involved in its provisions which are not always understood by botanists, especially by non-taxonomists, and not always appreciated by the taxonomists themselves.

One of these relates to the history of codes in general but especially to the so-called Paris Code of 1867, since it is the direct progenitor, in a figurative sense, of the modern code of 1935. The Paris Code was the first formulation of nomenclatural principles and rules for which the claim of internationality was made and to which adherence by all taxonomists was expected. In the Paris Code the principle of priority was the leading feature, just as it remains today. But those who care to study the code carefully and to inquire into the circumstances of that period which led to the appointment of De Candolle to draft the code will at once realize that absolute priority was not intended and that the effect of absolute priority was probably not imagined. If there was a conflict in the general usage of names in the various countries of western Europe (America apparently received little or no consideration), the choice of the conflicting names should depend on priority of publication, other things being equal. De Candolle never insisted on investigation of the merits of all published names: those that had already been relegated to the nomenclatural waste-basket were better left there undisturbed.

Other persons doubtless realized the potential danger in a strict interpretation of the rules. Some readers will remember the presidential address of L. H. Bailey before the American Society of Plant Taxonomists, in which he told of finding the Paris Code on the library shelves at Harvard and his proposal to Asa Gray that he (Bailey) translate them into English. To which Gray replied "Mr. Bailey, you will do no such thing. Let sleeping dogs lie."

Yet Asa Gray followed the principle of priority. If there was a choice to be made between two or more names, it was his prevailing practice to adopt the oldest. And so far as I know, he did so without mentioning them as the justification for his action.

About twenty years elapsed before anyone aroused the

sleeping dog. Nathaniel Lord Britton, my former professor and for many years my superior officer at the New York Botanical Garden, whose botanical ability, measured by his accomplishments, stands second to none in the country, first attempted to follow the provisions of the code beyond its original intent. In the late eighties and nineties he, sometimes alone and sometimes with assistants, hunted out hundreds of forgotten or discarded specific epithets, combined them with the valid generic names, and introduced the new combinations to the botanical public.

Of course there was a storm of protest, although Britton was right, according to the provisions of the current International Code. But the gates were now open and the floodwaters of nomenclature inundated the fields of taxonomy. After fifty years of drainage, after forty years of damming by nomina conservanda, those fields are still miry. Hardly an issue of *Rhodora* appears in which a change of name of some eastern American plant is not proposed, strictly in accordance with the code, of course. Some of these authors, who now stand on technicalities of the code, might well remember that their own predecessors were among the loudest in condemnation of Britton, who also was guided by similar technicalities in the code of his day.

The first attempt to restore nomenclature to some degree of sanity came with the codes of 1905 and 1910. In them there was no change from the early provision for the use of the oldest valid specific epithet, which was the prime cause of the trouble, but an attempt was made to reduce the effect of this provision. The use of tautonyms was abolished; epithets used in one category were not required to be transferred to another category; a number of generic nomina conservanda were adopted; a starting point later than 1753 was fixed for certain groups. Each of these provisions tended to restrict the damage caused by the discovery of unknown names or the revival of forgotten ones. All of them have been continued in the code of 1935 and the number of nomina conservanda has been increased.

Of late years a new dam has been opened, again to flood taxonomy. This is the problem of typification, not yet thoroughly controlled by the recent codes. The waters swirl round and round between *Quercus rubra* and *Quercus borealis*, between *Euphorbia maculata* and *Euphorbia supina*, leaving marooned and helpless the poor botanist who uses names as appellations for plants and not as botanical footballs.

Football players are invited to consider this: Nowhere in the Code is there any requirement that botanists should laboriously investigate encyclopedias, books of travel, textbooks of horticulture, and similar works and attempt to apply the names which they may find therein. It does not re-

quire that they find, investigate, typify, and apply every published binomial. The code does require that they use the oldest known legitimate epithet, not the oldest one as yet unknown. If they insist on looking up hitherto unknown names, then they should be consistent and investigate all encyclopedias, all books of travel, all textbooks of horticulture, all back volumes of the Congressional Record, all printed literature in every language, and thereby be sure that they have really found the oldest name.

The current code of nomenclature is intended to achieve a definite stated purpose; it is based on certain general principles; the use of these principles to attain the goal is implemented by a long series of rules.

The purpose is the establishment of a stable nomenclature. The rules do not distinguish between stability of the past and stability of the future. On the contrary, the rules clearly intend to maintain the stability of the past and to project it into the future. This is evidenced by the general principle that no one should change names except for serious reasons, by the use of different dates of departure, by the abolition of tautonyms, by the adoption of *nomina conservanda*, and (what may seem strange to some botanists) by the homonym rule, which often permits the segregation of a genus without the publication of a new generic name.

A careful study of the opening clauses of the Code will convince any impartial reader that the Code is intended to effect stability just as far as possible by maintenance of names and just as little as possible by change of names. The definite rules which follow and which constitute the bulk of the Code should therefore be used to justify maintenance. Only when maintenance is impossible should they be used to determine the nature of the necessary change.

Those who frequently turn to the pages of the Code for guidance and others who follow the current literature of taxonomy are fully aware that there are clauses of dubious application among the rules, rules which actually or seemingly conflict, nomenclatural problems connected with typification and hybridization which are not fully met. In all such cases, the rules should be interpreted to favor the maintenance of a name rather than its change. There are nomenclatural problems the settlement of which seems to depend on mere quibbling. I should not hesitate to quibble about the interpretation of a rule if by so doing I can preserve a well known name; I should quibble in the opposite direction with equal readiness if I can thereby preserve another name. If I can find any rule which will lead to the preservation of a name, I shall adopt it, although another rule may be found which would necessitate a replacement.

In general, if botanists will search as assiduously for

reasons to maintain a name as they do for reasons to change one, a considerable number of well known names will be saved.

I now present five instances of well known plants with names long established in the literature of botany, forestry, or horticulture which have come under recent criticism. In each case strange names or new combinations have been suggested for them. While I doubt that any change of name can "throw science into confusion," (International Code, Art. 3, paragraph 1) these plants are so common or so important that any change in their names should be avoided.

#### Parthenocissus vitacea.

It is only a half-century since the existence of two species of Virginia Creeper in our flora was noted. Apparently Lazenby was the first American botanist who in 1888 and 1890 called attention to the two forms, while Knerr gave the second one a varietal name in 1893. In doing so he emphasized the lack of adhesive disks on the tendrils and mentioned a few other subordinate features. A year later Hitchcock elevated Knerr's name to specific rank as Parthenocissus vitacea, under which name it has frequently appeared in American literature.

Recently Rehder has found another name, Vitis inserta Kerner, six years older than Knerr's variety and seven years older than Hitchcock's species. He accordingly transferred it to Parthenocissus and the plant appears as P. inserta in such widely used works as Rehder's Manual of Cultivated Trees and Shrubs (1940) and Deam's Flora of Indiana (1940); Fernald accepted it in Rhodora (43: 604. 1941), where he misspelled it as incerta.

Now let us examine Kerner's original publication. It consists of a figure and a bit of description. The figure shows what might be a bit of rock or a piece of bark, probably the latter, with two stems running vertically across it. Each stem has a palmately compound, 5-foliolate leaf, one of them with a tendril opposite it; the petiole of a third leaf is shown, also opposite a tendril. Anyone will recognize it as a Virginia Creeper and Kerner verifies this by referring to the plant as Vitis (Ampelopsis) inserta. Each tendril branches with four apices; each apex has found a crevice in the bark and has there enlarged into an adhesive disk. The drawing does not show the inflorescence, which is the best diagnostic character of the species, nor can it well show whether the leaf is dull or glossy. Kerner's description is not that of a taxonomist nor is there any evidence that he wished or intended to describe a species or propose a name, although this fact is in itself no reason for neglecting his name. He is writing about the be-

havior of tendrils and nothing else. Rather than quote the original German, I append an excellent translation by Oliver (Kerner & Oliver, Natural History of Plants 1: 701.):

"Bignonia capreolata, and Vitis (Ampelopsis) inserta (whose tendrils are represented in fig. 166<sup>1</sup>) behave differently from the three tendril-plants just mentioned. Here the curved tips of the tendrils, growing towards the wall, seek the crevices and crannies of stone or bark and actually creep into them, or when only shallow grooves are to be found in the substratum, bury themselves in them.\*\*\*When established in the chinks and crevices, the ends, which until now have been hooked, swell out like a club or ball, and in a short time thicken so much that they occupy the entire crack."

This is all the descriptive matter; the remainder of the paragraph deals in more detail with the adhesive properties of the tip of the tendril.

Kerner's description is not that of a taxonomist nor is there any evidence that he wished or intended to describe a species or propose a name, although this fact is in itself no reason under the Code for neglecting his name. He is writing about the behavior of tendrils and nothing else. The only structural feature of the plant to which reference is made is the tendrils and special emphasis is placed on the production of terminal hold-fasts.

That is precisely the feature which is used by modern botanists, including Rehder, to characterize P. quinquefolia! P. vitacea is the plant almost always without hold-fasts, and yet Rehder wants to displace that well known name by the one of Kerner. Vitis inserta, inadequately and accidentally although effectually published, is merely a synonym of P. quinquefolia.

#### Nelumbo lutea.

The American lotus has regularly been known by this name since 1805, while the specific epithet for it dates back to 1799. Recently Fernald has drawn attention to Nymphaea pentapetala Walt., published in 1788, and has advocated the new name Nelumbo pentapetala (Walt.) Fern.

Fernald has stated the facts correctly. Walter thought he had two species of Lotus. One of them he misidentified with the Old World species under the name Nymphaea Nelumbo. The other he regarded as undescribed; he gave it the specific name pentapetala and a brief description: "foliis peltatis undique integris, calyce pentaphyllo, corolla magna pentapetala alba, loculis pericarpium monospermis." Now the species of Nelumbo have numerous petals, not five. Walter's plant was either a monstrosity or an aged flower from which

the other petals had fallen. Fernald expressed regret at changing a well known name, but based his regret solely on the inappropriateness of the epithet. In this he was probably guided by Recommendation XIII: "The specific epithet should \*\*\* give some indication of \*\*\* the characters \*\*\* of the species," rather than by Article 15: "The purpose of giving a name to a taxonomic group is not to indicate the characters or history of the group, but to supply a means of referring to it." He could have avoided all regret if he had relied on Article 65: "A name or epithet of a taxonomic group must be rejected when it is based on a monstrosity." I regard Walter's name as covered by this rule and reject it accordingly.

Acer saccharum.

It is generally accepted as a fundamental principle of good nomenclature that the publication of a misprint does not produce a legal plant-name. There are all sorts of misprints which one may note in botanical works. Most of them are obvious, but there are some supposed cases which have been interpreted in two ways, as a misprint and as an intentional act.

Recently the botanical public has been asked to substitute Acer saccharophorum for A. saccharum as the name of our familiar northern Sugar Maple. The circumstances have already been treated in great detail and exactness by Rousseau (Contr. Inst. Bot. Univ. Montreal 35: 1--66. 1940.). He, however, wished to prove his own opinion and naturally presented all the evidence which he could develop in favor of it, while excusably slighting evidence to the contrary. Since his work may not be easily available to some readers, a very brief statement of the pertinent facts may be in order. Some of these facts are taken directly from Rousseau; others from the same literature from which Rousseau drew his evidence. No additional facts are necessary for proper appraisal.

Peter Kalm, in his travels in America, soon learned to know the Sugar Maple and collected specimens of it. Two of these are still extant. One, which came into the possession of Queen Louisa Ulrika, is Sugar Maple. A second, unfortunately sent to Linnaeus, is Silver Maple.

Linnaeus described four species of American maples in 1753, A. saccharinum, A. rubrum, A. pensylvanicum, and A. Negundo. Knowing what Kalm had learned about the Sugar Maple, and unaware of the confusion of the actual specimens, he supposed that Kalm's specimen represented that tree. He accordingly named it the "sugary maple", or A. saccharinum, and gave it one of his usual brief diagnoses. The descrip-

tion fits the specimen precisely, and both plant and description leave no room for doubt that the name A. saccharinum belongs to our Silver Maple. Succeeding botanists generally supposed, as Linnaeus had, that the name applied to the Sugar Maple and it was commonly used for that tree until 1889.

The next name given the Sugar Maple was Acer sacchatum by Philip Miller. It is generally supposed that this is a genuine misprint. Since there is no controversy, it needs no discussion here. The third name was Acer saccharum Marshall; a few others were given later, but since they are pure synonyms they also need no discussion.

In 1889 Britton and Sargent called attention to the misapplication of A. saccharinum. They proceeded to use that name for the Silver Maple and have been followed by almost all botanists since. For the Sugar Maple Britton brought up Marshall's name, A. saccharum, which was soon generally adopted and has been in common use by botanists and foresters for more than half a century.

Three decades more pass by and Mackenzie, always alert for an opportunity to make trouble in nomenclature, reported that saccharum, as originally used by Marshall, was merely a misprint for saccharinum. This drew mild protests from Sudworth and Sprague, and in general botanists continued to use saccharum, even down to the last edition of Rehder's Trees and Shrubs. Rehder is not particularly averse to a change of name; neither is Fernald, who also continued to use saccharum, although recently he has added saccharophorum in parentheses. Apparently neither was convinced by Mackenzie's argument. Rousseau, examining all pertinent literature and reporting it in meticulous detail, is convinced that saccharum is a misprint.

The only valid evidence must be taken directly from Marshall's Arbustum Americanum in which the name appeared. Let us put ourselves in Marshall's position, turning the calendar back 160 years. Encouraged by Bartram, we begin to write an account of the trees known to us in America. We have a good field knowledge of many of them. When we come to the maples, we note with astonishment that we have six different species, while the great Linnaeus himself had only four. Well, we shall do the best we can with them. Here is one described by Linnaeus as "Acer foliis compositis, floribus racemosis." This seems to fit our Box Elder, which is the only maple we have with compound leaves, and we write its name in our book as Acer Negundo. Correct.

Next we take up two of our plants, both small trees with flowers in racemes, and now we are baffled. Among his four species Linnaeus has only one which will fit, "Acer foliis trilobis acuminatis serrulatis, floribus racemosis." The des-

cription fits both of ours equally. We weigh every word of it and after due deliberation we finally apply the name to -- to the wrong species. Our A. spicatum of modern times appears as A. pensylvanicum L., while to the true A. pensylvanicum is given a new name, A. canadense.

Now we have two Linnean names left and three species still before us. A. rubrum is characterized by Linnaeus with "foliis quinquelobis subdentatis subtus glaucis, pedunculis simplicissimis aggregatis". Two of ours, the Red Maple and the Silver Maple, have a crowded inflorescence and leaves paler beneath. Again we consider the question carefully, note that A. rubrum has leaves "quinquelobis", and with some hesitation use that name for the Red Maple. This time we are correct, but we are not fully satisfied, for in our later description of the Silver Maple we hedge by writing "This is perhaps the Acer rubrum of Linnaeus."

There are still two species to be named and only one name available, A. saccharinum. This name seems to apply, by its meaning, to the Sugar Maple: did not Kalm tell us how sugar was made from it? But Linnaeus said the leaves were "quinquepartito-palmatis acuminato-dentatis", and nothing more, while the leaves of our tree would be described as "quinque-lobatis". Our Silver Maple has five-parted leaves, to be sure, but no one in Pennsylvania makes sugar from it. Besides its leaves are whitened beneath; why did not Linnaeus mention such a conspicuous character. The whitened surface leads us to doubt whether our Silver Maple may not be the Acer rubrum, but we have already decided to use that name for our Red Maple.

There is only one obvious solution, that we have two unnamed species. We proceed to describe our Silver Maple as A. glaucum, appropriately referring to the color of the leaves. Our Sugar Maple, with merely lobed leaves, is not the one which Kalm knew and Linnaeus described. Ours is a second species of Sugar Maple, and we name it by translating its local name directly into Latin, Acer saccharum.

That is no misprint. It is only an honest attempt by Humphrey Marshall to identify his plants according to the brief available descriptions written by a foreign botanist. It was an attempt correct in only two instances; an attempt which resulted in a misidentification for A. pensylvanicum, an attempt in which he failed to recognize in his own material any plants which corresponded to A. saccharinum L. and failed to find in literature any names which he felt he could properly use for the Silver Maple, the Sugar Maple, and the Moosewood. The total result was three supposedly new species.

Rousseau adduces one other fact as alleged proof of a misprint. Marshall's book was translated into French a few



years later by Lezermes and in the translation we find A. saccharum replaced by A. saccharinum. Rousseau believes this change was the correction of a misprint. We can more easily infer that the translator believed there was only one Sugar Maple in America and that it was an error in botanical judgement which required correction, not a misprint. Marshall gracefully bowed to European opinion and permitted the change. Such a correction, of course, can not void the validity of an earlier name.

Finally Rousseau states that A. saccharum, if not a misprint, becomes a nomen nudum, since there is no accurate means of deciding whether Marshall described the Sugar Maple or the Black Maple. The last clause of this sentence is undoubtedly true; the conclusion which he drew from it is erroneous. Rousseau implies by his statement that the name applies to one or the other of these maples. It might also apply to both, since both live in eastern Pennsylvania. If it applies to the Sugar Maple, it becomes the valid name for that species. If it applies to both species, it "must be retained for one of them, or (if it has not been retained) must be re-established" [Article 52]. Britton in 1889 considered that the name belonged to both species, and by naming the Black Maple A. saccharum var. nigrum he indicated that the typical nomenclatural element of the name applied only to the Sugar Maple. If the name applies only to the Black Maple, it has priority over and displaces A. nigrum Michx. (1803) but, since its application has been fixed by Britton's action and perpetuated by many years of usage, the burden of proof is upon those who might wish so to restrict it. Such proof has never been presented and probably can never be.

I therefore retain Acer saccharum as the valid name for the Sugar Maple.

#### Lathyrus maritimus vs. Lathyrus japonicus.

When Fernald discussed these names in 1932, he professed to regret that the International Code compelled the displacement of such a well known name as Lathyrus maritimus for such a well known plant as the Beach Pea.

The facts of the matter are simple and were well stated by Fernald. The Beach Pea lives on both Atlantic and Pacific shores of Eurasia and North America and also inland in suitable habitats. In spite of this broad distribution, it is regularly regarded as a single species. It was described from Europe by Linnaeus in 1753 as Pisum maritimum. It was described from Japan by Willdenow in 1803 as Lathyrus japonicus. It was described from Massachusetts by Bigelow in 1824 as Lathyrus maritimus. It was described from Scandinavia by

Fries in 1834 as Lathyrus maritimus. It has received other specific or subspecific epithets, none of which have any bearing on the present problem.

The earliest specific epithet is of course maritimus; the next is japonicus. Now here is the crux of the question. If Bigelow transferred the Linnean name from Pisum to Lathyrus in 1824 he then created a new and valid binomial, Lathyrus maritimus (L.) Bigel., which must stand as the name of the species. On the contrary, if Bigelow described a new species, then the transfer of the Linnean epithet to Lathyrus by Fries in 1834 merely created a homonym which is invalid under the International Code. Being invalid, the next oldest specific epithet must be used, which is japonicus.

Did Bigelow transfer an epithet, or did he describe a new species? Fernald, apparently looking for a reason to change a name, says a new species was described.

The essential purpose of the International Code is stated in Article 4. It is to strive for fixity in nomenclature. This purpose is implemented by the long series of rules and recommendations which constitutes the bulk of the code. If we are to strive for fixity of names, we must search the rules for clauses which will permit us to maintain a well known name. Fernald found clauses which permitted him to change a name. Are there other clauses which will authorize us to maintain the name? If Bigelow made a transfer, the name will automatically be maintained. Did he make such a transfer?

Some evidence on this point may be discovered by examining Bigelow's treatment of other species.

There are thirty species in his *Florula Bostoniensis* which are treated differently from the others, in that the usual diagnosis in English is preceded by a diagnosis in Latin. Of these thirty, twenty-three include no statement of synonyms of any kind, and are each preceded by an asterisk. Each of them represents the first publication of a new binomial (in one instance a trinomial) to designate what Bigelow believed to be a new species (in one instance a variety). Not all of them stand today, most of them having been previously described without Bigelow's knowledge or being otherwise untenable. The point is, that in describing a "new" species, he preceded the name by an asterisk and gave a Latin diagnosis. Five of the thirty are preceded by an asterisk, have a Latin diagnosis, but include some mention of synonyms. Bunias edentula is merely continued from its original publication in the first edition; the synonym, Cakile americana Nutt., is later than Bigelow's name. Galium Torreyi is new here as a species; its synonym is a variety, here raised to specific rank. Prunus obovata is a new species here, the synonym merely indicates that Pursh had con-

fused it with P. serotina. Prunus littoralis is also new; its synonym indicates that Michaux had confused it with P. sphaerocarpa. The fifth, Actaea alba, is followed by an explanatory note: "First published as a distinct species, in my name, in Eaton's Manual of Botany, afterward by Mr. Elliott under another name." The synonyms include Elliott's name and two varietal names under which the plant was treated by Michaux and Pursh. Considering these five with the preceding twenty-three, we are at once led to the conclusion that every species or specific name for which Bigelow was responsible was so designated by an asterisk

There are still two left over which have a Latin diagnosis but no asterisk. The first of these is Iris prismatica Pursh, a plant "first described by me in the former edition of this work under the name of I. gracilis. Two years afterwards Mr. Pursh gave it the name of I. prismatica, which name I am willing to adopt." The other is Lathyrus palustris, under which he cites "Syn. Pisum maritimum. Pursh?" In both cases the absence of an asterisk indicates a species for which Bigelow is not responsible.

We can easily interpret Pisum maritimum as the basonym, and we shall do so if we are seriously interested in the spirit of the International Code. It was not necessary to cite the original author of the name (Linnaeus); there was no other Pisum maritimum with which it could be confused. Citation of authors is for "purposes of precision" [Code, Sect. 7] and "in order that the date may be readily verified" [Article 46]. Article 44 states that "the name of a species \*\*\* is not validly published unless it is accompanied \*\*\* by the citation of a previously and effectively published description \*\*\* under another name." The mention of Pursh can be construed to cover this requirement. The Code does not specifically require the mention of volume and page.

The case is closely parallel to that of Hedysarum glutinosum Willd. (1802) and Desmodium glutinosum Wood (1845). Both names apply to the same species. If Wood's name is a transfer of Willdenow's oldest specific epithet, it becomes the valid binomial for the species. If on the other hand it is a description of a new species, its existence invalidates the later transfer of Willdenow's name to Desmodium by Schindler (1926) and necessitates the revival of the next oldest specific epithet, acuminatum Michx. (1803), in the well known binomial Desmodium acuminatum (Michx.) DC. Miss Schubert [Rhodora 44: 279] says: "Although it is true that Wood cited neither authority nor synonyms his description leaves no doubt as to his intention nor as to the identity of the plant he was considering." Here she has done precisely what Fernald refused to do for the Beach Pea and done it

probably with Fernald's knowledge and possibly with his approval. The adoption of opposite opinions for the two plants has permitted them to recommend the abandonment of two well known names.

And Fernald himself has done the same thing. In *Rhodora* 44: 424 he takes up the name Rhynchosia difformis (Ell.) DC. He says "Although DeCandolle failed to cite the synonym Archyphyllum difforme Ell., the diagnosis \*\*\* and the habitat \*\*\* are so clearly derived from Elliott that the combination should certainly be written Rhynchosia difformis (Ell.) DC."

In each of these three cases we admit the conspecificity of the plants involved and we know the source of the specific epithet used in the combination. Bigelow is the only one who cites the name-bringing synonym; Bigelow also shows by his typography that he did not regard his name as designating a new species, a change of name, or a replacement of an untenable name. How else do valid names arise except by transfer?

Following the spirit and intent of the Code, taking advantage of loopholes in Article 44, and imitating the precedent of Schubert and Fernald, I shall maintain the well known and long established name Lathyrus maritimus (L.) Bigel. for the Beach Pea.

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### A NEW SPECIES OF DAPHNOPSIS FROM ECUADOR

Joseph V. Monachino

DAPHNOPSIS ESPINOSAE Monachino, sp. nov.

Arbuscula; foliis ellipticis ca. 4--8 cm. longis et 1.5--3 cm. latis glaberrimis; petiolis 3--4 mm. longis, 1.5 mm. latis; inflorescentiis caulifloris 1.5--2 cm. longis; floribus femineis 6--12 subumbellato-racemosis; calyce campanulato, ca. 2.5 mm. longo, extus parce pubescente, lobis rotundatis ca. 1.5 mm. longis paullo latioribus intus pubescentibus; staminodiis et petalorum rudimentis nullis; ovario glabro; stylo 0.8 mm. longo; stigmatе capitato exserto; disco crateriformi irregulariter lobato glabro.

Vegetative parts completely glabrous except for the ciliate bud-scales; petioles about 3 or 4 mm. long and 1.5 mm. broad; leaf-blades glabrous on both surfaces from the beginning, becoming chartaceous or subcoriaceous and shining above, elliptic, narrowed at both ends, obtuse or acute at apex, 4--8 cm. long and 1.5--3 cm. broad, the reticulation prominulous; inflorescences cauliflorous, 1.5--2 cm. long,